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Einstein Versus the *Physical Review*

A great scientist can benefit from peer review, even while refusing to have anything to do with it.

Daniel Kennefick

Albert Einstein had two careers as a professional physicist, the first spent through 1933 entirely at German-speaking universities in central Europe, the second at the Institute for Advanced Studies in Princeton, New Jersey, from 1933 until his death in 1955. During the first period he generally published in German physics journals, most famously the *Annalen der Physik*, where all five of his celebrated papers of 1905 appeared.

After relocating to the US, Einstein began to publish frequently in North American journals. Of those, the *Physical Review*, then under the editorship of John Tate (pictured in figure 1), was rapidly assuming the mantle of the world's premier journal of physics.[1] Einstein first published there in 1931 on the first of three winter visits to Caltech. With Nathan Rosen, his first American assistant, Einstein published two more papers in the *Physical Review*: the famous 1935 paper by Einstein, Boris Podolsky, and Rosen (EPR) and a 1936 paper that introduced the concept of the Einstein–Rosen bridge, nowadays better known as a wormhole. But except for a letter to the journal's editor he wrote in 1952—in response to a paper critical of his unified field theory work—that 1936 paper was the last Einstein would ever publish there.

Einstein stopped submitting work to the *Physical Review* after receiving a negative critique from the journal in response to a paper he had written with Rosen on gravitational waves later in 1936.[2] That much has long been known, at least to the editors of Einstein's collected papers. But the story of Einstein's subsequent interaction with the referee in that case is not well known to physicists outside of the gravitational-wave community. Last March, the journal's current editor-in-chief, Martin Blume, and his colleagues uncovered the journal's logbook records from the era, a find that has confirmed the suspicions about that referee's identity.[3] Moreover, the story raises the possibility that Einstein's gravitational-wave paper with Rosen may have been his only genuine encounter with anonymous peer review. Einstein, who reacted angrily to the referee report, would have been well advised to pay more attention to its criticisms, which proved to be valid.

Doubting gravitational waves

Einstein introduced gravitational waves into his theory of general relativity in 1916, within a few months of finding the correct form of the field equations for it. Although the concept of gravitational radiation was then relatively new and no experimental evidence existed to support it, the analogy with the case of the electromagnetic field was so compelling that by the 1930s most scientists thought that gravitational waves must exist in principle. Nevertheless, in 1936 Einstein wrote to his friend Max Born:

> Together with a young collaborator, I arrived at the interesting result that gravitational waves do not exist, though they had been assumed a certainty to the first approximation. This shows that the non-linear general relativistic field equations can tell us more or, rather, limit us more than we have believed up to now.[4]

Einstein submitted this research to the *Physical Review* under the title "Do Gravitational Waves Exist?" with Rosen as coauthor. Although the original version of the paper no longer exists, Einstein's answer to the title question, to judge from his letter to Born, was "No." It is remarkable that at this stage in his career Einstein was prepared to believe that gravitational waves did not exist, but he also managed to convince his new assistant, Leopold Infeld, who replaced Rosen in 1936, that his argument was valid.[5] Infeld is shown with Einstein in figure 2.

But not everyone was so easily convinced. The *Physical Review* received Einstein's submission on 1 June 1936, according to the journal's logbook. Tate returned the manuscript to Einstein on 23 July with a critical review and the mild request that he "would be glad to have [Einstein's] reaction to the various comments and criticisms the referee has made." Einstein wrote back on 27 July in high dudgeon, withdrawing the paper and dismissing out of hand the referee's comments:

> Dear Sir,
>
> We (Mr. Rosen and I) had sent you our manuscript for publication and had not authorized you to show it to specialists before it is printed. I see no reason to address the—in any case erroneous—comments of your anonymous expert. On the basis of this incident I prefer to publish the paper elsewhere.
>
> *Respectfully,*
>
> P.S. Mr. Rosen, who has left for the Soviet Union, has authorized me to represent him in this matter.

On 30 July, Tate replied that he regretted Einstein's decision to withdraw the paper, but stated that he would

Daniel Kennefick *is a visiting assistant professor of physics at the University of Arkansas at Fayetteville and an editor with the Einstein Papers Project at the California Institute of Technology.*

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not set aside the journal's review procedure. In particular, he wrote, "I could not accept for publication in THE PHYSICAL REVIEW a paper which the author was unwilling I should show to our Editorial Board before publication."

The paper was, however, subsequently accepted for publication by the *Journal of the Franklin Institute* in Philadelphia,[6] a periodical in which Einstein had already published. The paper appeared with radically altered conclusions in early 1937. A letter dated 13 November 1936, from Einstein to the journal's editor, indicates that the institute had accepted the paper in its original form: Einstein simply explained why "fundamental" changes in the paper were required because the "consequences" of the equations derived in the paper had previously been incorrectly inferred.

What originally led Einstein to the conclusion that gravitational waves do not exist? Having set out to find an exact solution for plane gravitational waves, he and Rosen found themselves unable to do so without introducing singularities into the components of the metric that describes the waves. This was surely not at all what they had hoped for. But, like good physicists confronted with the unexpected, they attempted to turn it to their advantage. In fact, they felt they could show that no regular periodic wavelike solutions to the equations were possible.[7] Instead of a solution to the Einstein equations, they had a nonexistence proof for solutions representing gravitational waves—a far more important and breathtaking result.

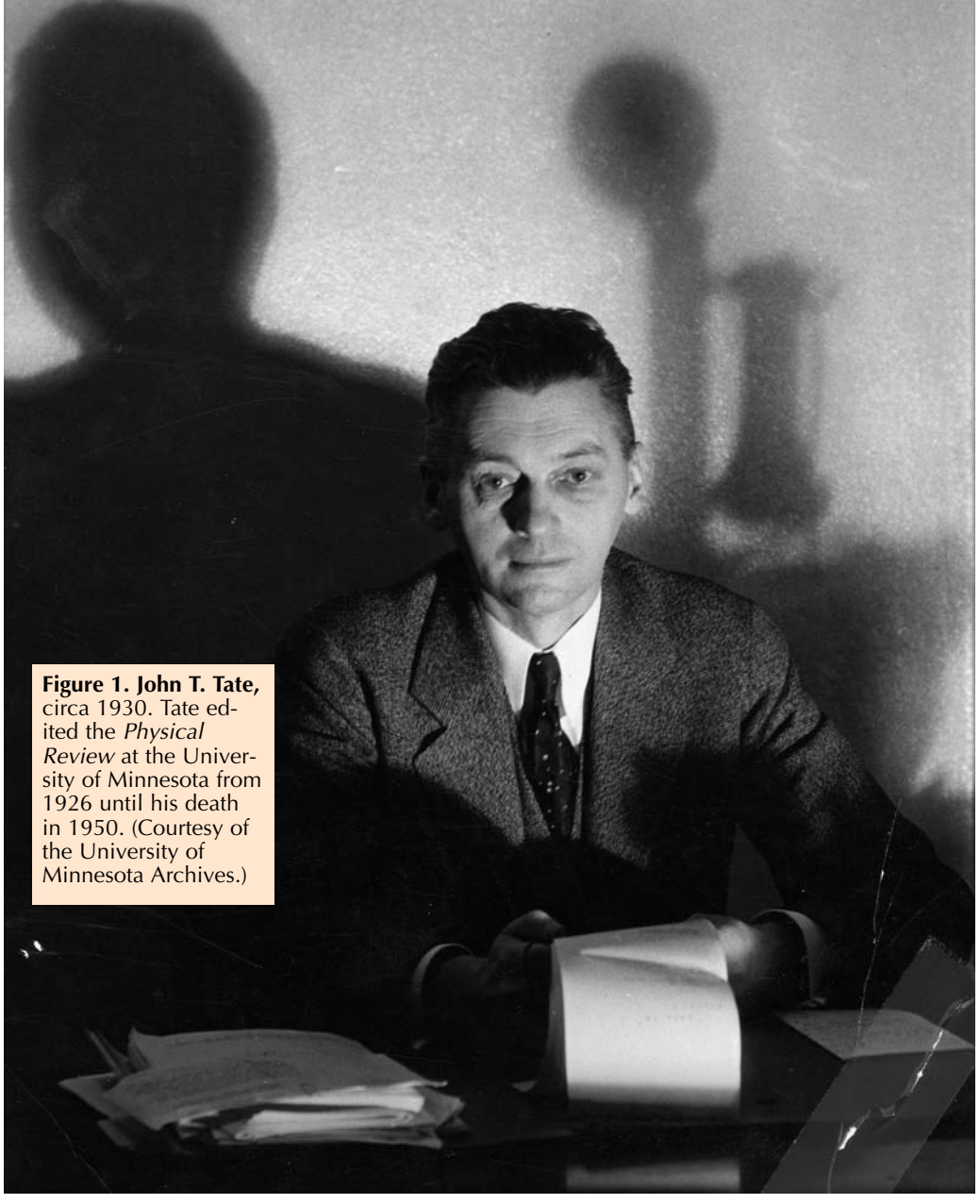

Figure 1. John T. Tate, circa 1930. Tate edited the *Physical Review* at the University of Minnesota from 1926 until his death in 1950. (Courtesy of the University of Minnesota Archives.)

Today it is well known that one cannot construct a single coordinate system to describe plane gravitational waves without encountering a singularity somewhere in spacetime. But it is also understood that such a singularity is merely apparent and not real. It is a coordinate singularity, analogous to the problem one encounters when attempting to find the longitude of the North Pole. Einstein was one of the first to understand the critical difference between coordinate and physical singularities, but in the 1930s there was still no mathematical formalism for distinguishing between the two. It was something that had to be worked out by trial and, frequently, error. Only after World War II did the identification of singularities become rigorous. In 1936 Einstein and Rosen were too cautious, treating a harmless coordinate effect as a real physical pathology. It simply did not occur to them that trying to cover the whole of their spacetime with a single coordinate system was asking too much.

Chance meeting

In the summer of 1936, the relativist Howard Percy Robertson (pictured in figure 3) returned to Princeton from a sabbatical in Pasadena, and later that year struck up a friendship with Einstein's then newly arrived assistant Infeld. One of the most distinguished figures in the new field of cosmology, Robertson was a colorful, jovial character who enjoyed cultivating enemies as much as he, in Infeld's words, "enjoyed spiteful gossip" about his colleagues.

He told Infeld that he did not believe Einstein's result, and his skepticism was unshakable. Robertson went over Infeld's version of the argument with him, and they discovered an error.[5] Infeld related the conversation to Einstein, who concurred and drastically changed the Franklin Institute paper in proofs.

Curiously, Infeld states that when he told Einstein he and Robertson had uncovered an error in his (Infeld's) version of the proof, Einstein replied that he had coincidentally and independently uncovered an error in his own proof the night before.[5] Unfortunately Infeld gives no details about those errors in his autobiography. He writes that Einstein had only realized that his proof was incorrect and had still not managed to find the gravitational wave solution he had been looking for.

But Einstein had been closer to a solution than he thought and it was here that Robertson made his key contribution, at least according to remarks made by Rosen in

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Figure 2. Albert Einstein and Leopold Infeld in Einstein's Princeton, New Jersey, home in 1938. (Courtesy of the Lotte Jacobi Collection, University of New Hampshire.)

a later paper of 1955.[8] Robertson observed that the singularity could be dealt with by a change of coordinates, an approach that revealed that Einstein and Rosen were dealing with a solution representing cylindrical waves. With the coordinate change the worrisome singularities were relegated to the central axis of the spacetime, where one would expect to find the source of the cylindrical waves.

Associating singularities with a material source was relatively common and widely accepted, although Einstein and some others had often expressed serious reservations about the practice. But any port in a storm will do, and Einstein was happy to retitle his paper "On Gravitational Waves," as shown in figure 4, and present those cylindrical waves, which he had stumbled upon unwittingly.

The irony, of course, is that Einstein could have found that escape route months earlier, simply by reading the referee's report that he had dismissed so hastily. The referee had also observed that casting the Einstein–Rosen metric (as we now call this solution of the Einstein equations) in cylindrical coordinates removes the apparent difficulty.

Coincidentally, in the Soviet Union, Rosen was also having second thoughts, and wrote back to Einstein that he, too, thought there was an error in the paper. But Rosen was not completely happy with the Franklin Institute version, so in 1937 he published his own revised treatment—one that proves only the nonexistence of plane gravitational waves—in a Soviet journal.[7] That paper is the closest account we have to the original manuscript submitted to the *Physical Review*. After the war, Ivor Robinson, Hermann Bondi, and Felix Pirani showed that Rosen's argument was incorrect because the singularities involved were merely coordinate in nature.

Meanwhile, Einstein was not a man to waste time on embarrassment. Infeld relates the amusing detail that Einstein was due to give a lecture in Princeton on his new nonexistence proof, just one day after his discovery of its errors. He had not yet spoken to Robertson and discovered the way out of his difficulty, and so was obliged to lecture on the invalidity of his own proof. He concluded the talk by saying "If you ask me whether there are gravitational waves or not, I must answer that I do not know. But it is a highly interesting problem."[5]

Einstein rarely let personal pride interfere with his work. While they were working on the popular book, *Evolution of Physics: The Growth of Ideas from Early Concepts to Relativity and Quanta*, which they wrote together, Infeld told Einstein that he took special care because he could not "forget that your name will appear on it."

> Einstein laughed his loud laugh and replied: 'You don't need to be so careful about this. There are incorrect papers under my name too.'[5]

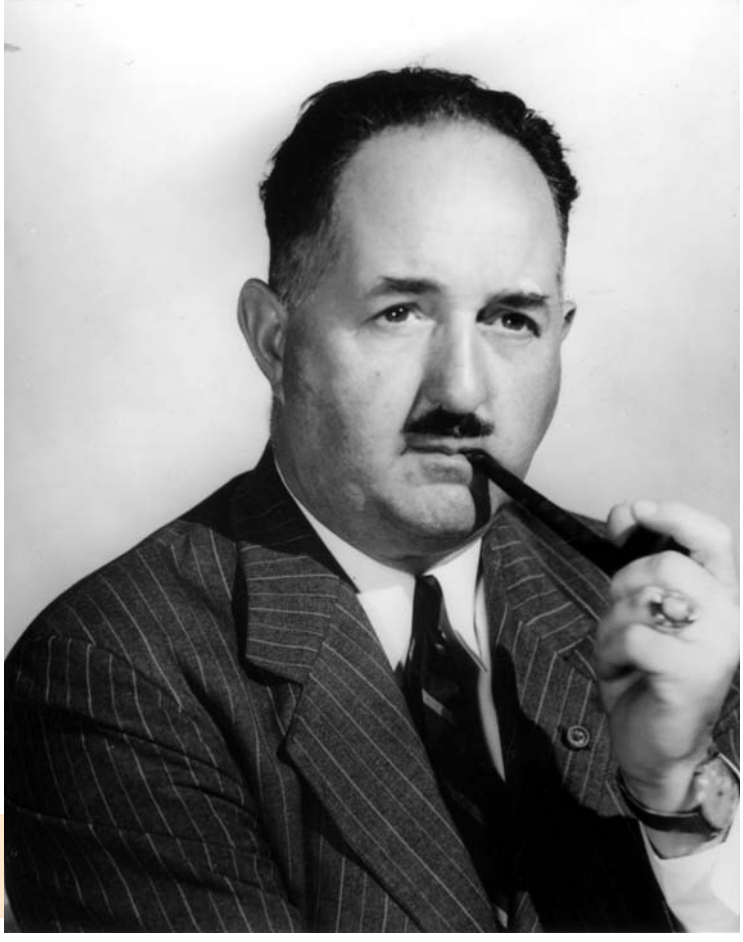

Figure 3. Howard Percy Robertson (1903–1961). (Courtesy of AIP Emilio Segrè Visual Archives, PHYSICS TODAY Collection.)

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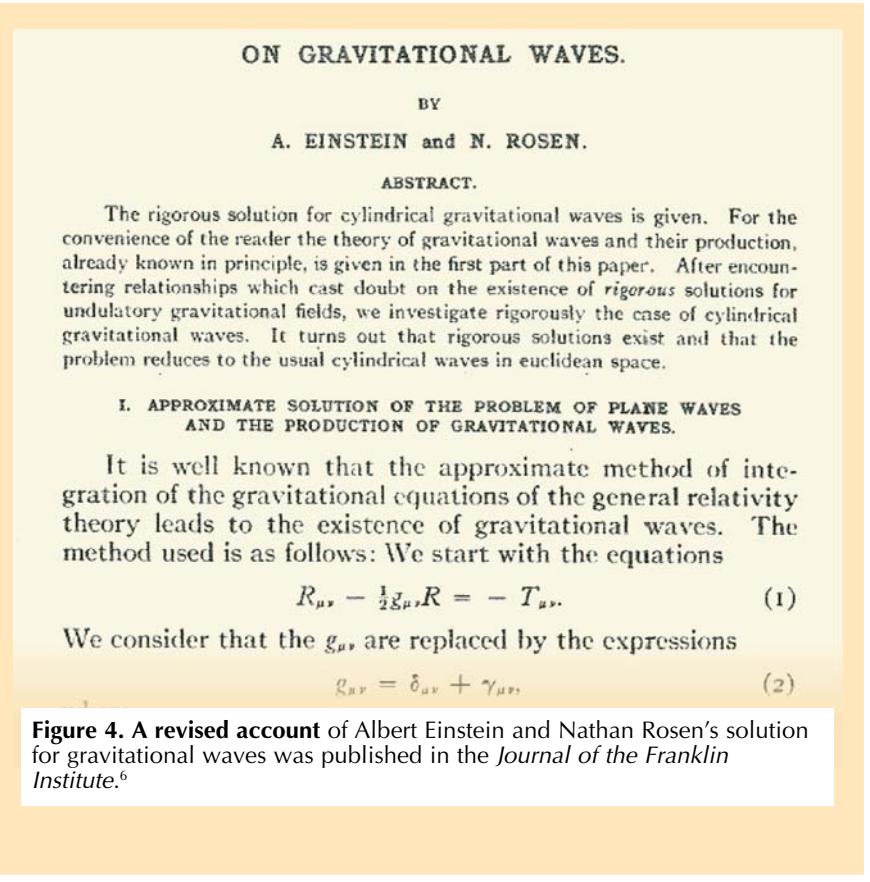
Referees and precedents

Although it now bears Einstein and Rosen's names, the solution for cylindrical gravitational waves had been previously published by the Austrian physicist Guido Beck in 1925. But Beck's paper was completely unknown to relativists with the single exception of his student Peter Havas, who entered the field in the late 1950s. In a 1926 paper by the English mathematicians O. R. Baldwin and George B. Jeffery, and in the referee's report on Einstein's paper, there was discussion of the fact that singularities in the metric coefficients are unavoidable when describing plane waves with infinite wavefronts. But although such a wave shows some distortion, in the words of the referee, "the field itself is flat" at infinity.[9]

Clearly, the referee's familiarity with the literature exceeded Einstein's, but then Einstein was notoriously lax in that regard. The published Einstein–Rosen paper contains no direct reference to any other paper whatsoever and only two other authors are even mentioned by name. In response to Infeld's suggestion that he search the literature for previous work, Einstein laughed and said, "Oh yes. Do it by all means. Already I have sinned too often in this respect."[5]



ON GRAVITATIONAL WAVES.

BY

A. EINSTEIN and N. ROSEN.

ABSTRACT.

The rigorous solution for cylindrical gravitational waves is given. For the convenience of the reader the theory of gravitational waves and their production, already known in principle, is given in the first part of this paper. After encountering relationships which cast doubt on the existence of *rigorous* solutions for undulatory gravitational fields, we investigate rigorously the case of cylindrical gravitational waves. It turns out that rigorous solutions exist and that the problem reduces to the usual cylindrical waves in euclidean space.

I. APPROXIMATE SOLUTION OF THE PROBLEM OF PLANE WAVES AND THE PRODUCTION OF GRAVITATIONAL WAVES.

It is well known that the approximate method of integration of the gravitational equations of the general relativity theory leads to the existence of gravitational waves. The method used is as follows: We start with the equations

$$R_{\mu\nu} - \tfrac{1}{2}g_{\mu\nu}R = -T_{\mu\nu}. \qquad (1)$$

We consider that the $g_{\mu\nu}$ are replaced by the expressions

$$g_{\mu\nu} = \delta_{\mu\nu} + \gamma_{\mu\nu}, \qquad (2)$$

Figure 4. A revised account of Albert Einstein and Nathan Rosen's solution for gravitational waves was published in the *Journal of the Franklin Institute*.[6]

So who was the referee? The report is 10 pages long and shows a deep, if not total, familiarity with the literature on gravitational waves; the referee knew of the 1926 paper by Baldwin and Jeffery, but not Beck's of 1925. The copy forwarded to Einstein was typewritten and the spelling followed American practices. That points to an American author with a strong interest in general relativity. Few people at the time—among them Robert Oppenheimer and Richard Chase Tolman, both based in California—fit that description. Suspicion naturally falls on Robertson too, of course. After all, he appeared to have the solution to the paper's flaws at his fingertips in the fall of 1936 when he spoke with Infeld.

In the first half of 1936, Robertson was on sabbatical at Caltech, and therefore absent from Princeton when the gravitational-wave paper was presumably written. (Rosen did not leave for the Soviet Union until near the end of July, according to a letter written on his behalf by Einstein to Vyacheslav Molotov on 4 July.) Robertson apparently did not return to Princeton until mid-August. Einstein was on vacation in upstate New York until late August; the angry letter to Tate, dated 27 July, was sent from Saranac Lake. Therefore Robertson's encounter with Infeld, which probably took place in early October, may have been his first opening to approach the great man in person about the difficulties with his paper.

Robertson's own papers are preserved in the Caltech archives. Among them, when I first browsed the collection ten years ago, was a letter to Tate, written on 18 February 1937. Robertson writes,

> You neglected to keep me informed on the paper submitted last summer by your most distinguished contributor. But I shall nevertheless let you in on the subsequent history. It was sent (without even the correction of one or two numerical slips pointed out by your referee) to another journal, and when it came back in galley proofs was completely revised because I had been able to convince him in the meantime that it proved the opposite of what he thought.
>
> You might be interested in looking up an article in the Journal of the Franklin Institute, January 1937, p. 43, and comparing the conclusions reached with your referee's criticisms.

Therefore, it seems clear that Robertson was the referee. Finding that Einstein had completely ignored his written critique, he took the opportunity of their collegial closeness at Princeton to correct the great man in a less confrontational fashion. Blume's release of the logbook records—a decision made because 69 years have passed and no one involved is still living—confirms the identity (see figure 5).

Inspired by this discovery, I returned to the Robertson archives to check on his movements that summer. To my surprise, further material had been added to the archive: Sitting in the middle of the Tate correspondence was most of the immediate exchange between Robertson and Tate concerning the Einstein–Rosen manuscript. Here is what Robertson had to say in his reply (dated 14 July) to Tate's still-missing original letter:

> Dear Tate:
>
> Well, this is a job! If Einstein and Rosen can establish their case, this would constitute a most important criticism of the general theory of relativity. But I have gone over the whole thing with a fine-tooth comb (mainly for the

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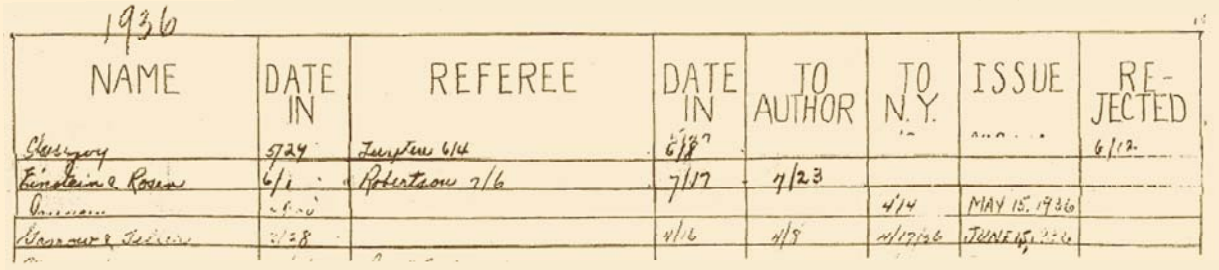

1936

NAME	DATE IN	REFEREE	DATE IN	TO AUTHOR	TO N.Y.	ISSUE	RE-JECTED
Gherzony	5/24	Tupton 6/4	6/8				6/12
Einstein & Rosen	6/1	Robertson 7/6	7/17	7/23			
O.....					4/14	MAY 15. 1936	
Vanvleck & Sessions	5/28		4/6	4/9	4/17/36	JUNE 15, 1936	

Figure 5. An early extract from the *Physical Review* logbook. The Einstein–Rosen article was received by the journal on 1 June 1936. After a delay of more than a month, John Tate sent a referral to Howard Percy Robertson on 6 July, finding him in Moscow, Idaho, on vacation after a sabbatical at Caltech. Robertson returned the manuscript and his review to Tate on 17 July. Six days later the package was sent back to Einstein. (Courtesy of Martin Blume, American Physical Society.)

> sake of my own soul!), and can't for the life of me see that they have established it. It has long been known that there are difficulties in attempting to treat infinite plane gravitational disturbances in general relativity—even in the classical theory the potential acts up at infinity in such cases—and as far as I can see the additional, much more serious, objections of Einstein and Rosen do not exist. I can only recommend that you submit my criticisms to them for their consideration, and with this in mind I have written up in duplicate a series of "Comments" which you can, if you are so minded, send them. The alternative would be to publish it as it stands, taking account only of Comments (a) and (b) which deal with typographical errors of a minor sort. Such a paper would be certain to give rise to a lot of work in this field of gravitational waves, which might be a good thing—provided they didn't flood you out of house and home.

Tate thanked Robertson and rewarded his diligent referee in the usual manner—by sending him another tricky assignment.

Early journal policies

We are probably justified in assuming that Einstein, overcome with the novelty of receiving such a report, barely glanced at the 10-page set of referee comments he was sent. German journals in the early part of the 20th century were considerably less fastidious than the *Physical Review* about what they published. Infeld claimed that the German attitude, in contrast to that prevailing in Britain and America, was "better a wrong paper than no paper at all."[5] In a March 1936 letter to Einstein, the relativist and fellow European exile Cornelius Lanczos, who had himself been on the receiving end of one of Robertson's reports, remarked on "the rigorous criticism common for American journals" such as the *Physical Review*.[10]

Historians Christa Jungnickel and Russel McCormmach have studied in some detail the editorial policies of *Annalen der Physik*, the leading German journal of the early 1900s, and note that "the rejection rate of the journal was remarkably low, no higher than five or ten percent."[11] They describe the editors' reluctance to reject papers from established physicists, even relatively junior ones. As they put it, "Now and then the journal published bad papers by good physicists." In one specific example, editor Paul Drude annoyed Max Planck by printing what Planck considered a worthless paper, whose author had "appealed to [Drude] personally, and Drude lacked the heart to refuse him."[11]

Planck's own editorial philosophy was to "shun much more the reproach of having suppressed strange opinions than that of having been too gentle in evaluating them."[10] In America things were different, although Robertson and Tate surely treated Einstein more gently than they would have many others. Indeed, Robertson, in his very next report to Tate, commented that the author "is a man of good scientific standing, and it would seem to me that if he insists, he has more right to be heard than any single referee has to throttle!" That dispute turned more on matters of interpretation, though, and when it came to a paper that might actually be wrong, even an Einstein had to be queried, however gently.

Doubtless the rigorous criticism may have come as something of a shock to Einstein, who was accustomed to gentler treatment early in his career. However, Einstein could be very frank and direct in his criticism of others' work. From 1914 on, as a member of the Prussian Academy of Sciences, he was regularly called on to review articles submitted to the academy's proceedings. The German word for worthless frequently occurs in those brief reviews. As a member of the academy, Einstein had his papers published without question or revision. Anything less must have seemed to him a tremendous slight.

In his letter to Einstein, Tate had carefully avoided stating that anonymous review by the editorial board or others was a necessary step in the acceptance of a paper by the journal. In fact, the *Physical Review* logbook suggests that neither of the two previous papers by Einstein and Rosen, including the one with Podolsky, had been sent to a referee: In both cases the field for the referee's name was left blank, and the EPR paper was sent for publication the day after its receipt at the journal. Therefore it is likely that the gravitational wave paper was Einstein's first encounter with the anonymous peer-review system practiced in American journals at the time.

That Tate chose to have the 1936 paper refereed is interesting. After all, Einstein's two previous submissions were certainly controversial. EPR is arguably the most

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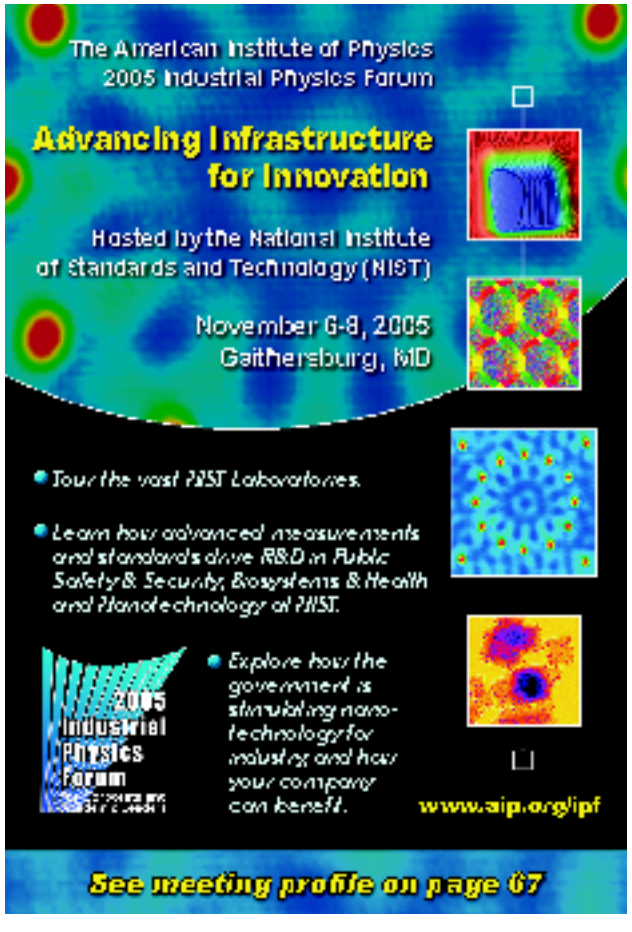

controversial paper Einstein ever published, and the Einstein–Rosen bridge paper was part of an ongoing controversy with Ludwig Silberstein.[10] Einstein and Rosen's letter to the *Physical Review* in 1935 was part of this same debate. Tate published both of those papers without outside advice.

A paper purporting to prove that gravitational waves did not exist, though, apparently sounded alarms with him. Nowadays one imagines that most physicists of the time knew little and cared even less about general relativity. But apparently gravitational waves were already such a well-accepted prediction of the theory, despite the absence of experimental support, that such a surprising result warranted some scrutiny. More than a month elapsed between receipt of the paper and its referral to Robertson. The delay certainly suggests hesitation on Tate's part, and may even be evidence of an initial round of editorial discussion.

In general Tate did not like to slow the publication of important work and often relied on his own editorial instincts,[12] which certainly served Einstein well. Tate published the better-known papers expeditiously and, by consulting Robertson for the third, saved Einstein from what would have been a very public embarrassment. The relatively innocuous Franklin Institute paper still attracted newspaper attention. Indeed, Rosen learned that the paper had appeared only when he received a newspaper clipping about it from a friend. The price for Tate was that he would never again receive a submission from "his most distinguished contributor."

Special thanks go to Martin Blume and the Physical Review *for permission to see and publish the critical line and details from the logbook. Also thanks to Diana Buchwald for translation of Einstein's letter to Tate, and to John T. Tate Jr for permission to quote from his father's correspondence. I am grateful to the Caltech Archives for permission to quote from the correspondence of H. P. Robertson and to the Hebrew University of Jerusalem for permission to quote from Einstein's correspondence.*

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